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THE RELAPSE RATE IN NARCOTIC ADDICTION: A CRITIQUE OF FOLLOW-UP STUDIES

by

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Lawrence W. Pierce

THE RELAPSE RATE IN NARCOTIC ADDICTION: A CRITIQUE OF FOLLOW-UP STUDIES

NARCOTIC ADDICTION is a controversial field. Conflicting views are current with regard to the extent of the problem, its etiology, treatment, the desirability of stricter or more lenient laws, whether addiction should be regarded as a crime or disease, and almost any other aspect of the problem one might isolate. There is, however, almost universal agreement, even among persons who strongly disagree on everything else, that the relapse rate among addicts is high—though “high” is rarely defined in numerical terms and may have different meanings for different people.^{10,11,13,15,16}

Originally, the conviction that relapse rates are high seems to have been based on studies of addicts in hospitals and prisons, of those arrested, or, in a few cases, of addicts introduced to the investigator by other addicts. When histories are taken in such situations, they will usually contain references to previous periods of treatment, or at least of withdrawal, and of subsequent relapse. The only histories which will not show relapse will be those of patients who have used the drug steadily from the first time they were addicted, who, in short, have had no opportunity to relapse.

The conviction is also based on the experience of those who have had contact with addicts over some period of time, either as therapists or law enforcement officers. They find themselves treating again, or arresting again, many of the addicts they had treated or arrested years before.

Psychologically, repeated experiences of these kinds explain why so many feel that relapse is almost inevitable in any case of addiction. But logically, of course, such experiences do not justify that conclusion. The circumstances in which retrospective studies are done make it certain that they will find only histories of relapse. If it be accepted, as a logical possibility, that there are some addicts who do give up their drug use and never relapse, then obviously such addicts do not get into these studies, and they have no chance to be counted. Similarly, the fact of relapse is brought home to law enforcement and treatment personnel by those addicts whom they see again and again, and they find it easy to forget those whom they do not see again.

Not all succumb to the logical fallacy. Even 35 years ago, Kolb⁷ called attention to the fact that many addicts sincerely try to abstain and implied that many must succeed. For years the late Dr. Kenneth Chapman called attention to the fact that current arrests and admissions of addicts in the older-age ranges fall far short of the figure we would expect if all the known youthful addicts of 20 or 30 years ago had continued to use drugs.

To this writer's knowledge, however, the literature contains only one statement that any sizeable, or specific, percentage of addicts can be expected to remain abstinent indefinitely or for a lengthy period of time. The one exception comes from Winick's use of Bureau of Narcotics files to check Chapman's suggestion, resulting in his conclusion "that addiction may be a self-limiting process for perhaps two-thirds of addicts."¹⁹

This suggestion is still highly tentative, however, as Winick is careful to point out, because of inherent limitations in the data currently available to the Bureau. The questions about relapse can only be answered by careful follow-up studies, studies which select a sample of persons who were addicted and withdrawn at some point in time and then, after some lapse of time, determine

their subsequent history of drug use or abstinence. This paper will consider the 11 follow-up studies of American narcotic addicts, which are, as far as can be determined, all the studies which have been reported. Its purposes will be to summarize what has been learned from these studies, with some attention to the limitations of the studies and the generalizations which may be inferred from them, and to suggest improvements in the design of future follow-up studies.

One prefatory note is in order. There is much that can be criticized in almost all these studies, but nothing to be said here is intended to detract from the investigators. These are pioneering studies, and one should not expect of them all the rigor and technical sophistication to be expected in an established field of investigation. Most of the investigators were not primarily oriented to research, but to treatment or program administration. Most of the studies were conducted as part-time projects by persons whose primary responsibilities lay elsewhere. They deserve credit for the research they did accomplish and little or no blame for the flaws which were probably inevitable. Further, it will be the better, more adequate studies which are most criticized, because only such studies furnish the critic with the ammunition needed to attack them.

FINDINGS OF THE STUDIES

The studies are summarized in Table 14-1. It should be noted that although most of the studies used "relapsed" and "abstinent," or equivalent terms, as the major classifications for their findings, some used other classifications. In the column of findings, their classifications have been translated, when possible, into "relapsed" and "abstinent." This operation may have introduced some error into the table. The notes in the bibliography describe how these translations were made.

Perhaps the outstanding point to be noted is the wide range of rates reported, from a high of 90 per cent relapsed found in one study, to a high of 92 per cent abstinent reported in another. So great a difference points to the possibility of errors, biases, and major differences in definitions of terms. Such explanations must

Table 14-1 FINDINGS OF FOLLOW-UP STUDIES

| Author | Sample size | Source of sample | Follow-up period | Findings (Percentage) | | | | |
|-------------------------------|-------------|---------------------------------------|------------------|---|---------------|------|---------|------------------------------|
| | | | | Relapsed | Irregular use | Dead | Unknown | Absent |
| Senate ¹⁷ | 584 | Committed patients, Spadra | 2 years | 85.1 | ... | ... | ... | 14.9 |
| Pescor ¹⁸ | 4,766 | Lexington | 6-72 months | 39.9 | ... | 7.0 | 39.6 | 13.5 |
| Knight ⁹ | 50 | Voluntary patients, New York Hospital | 1-21 years | 33.3 | ... | 17.6 | 27.5 | 21.6 |
| Kuznesof ⁸ | 83 | Lexington-New York probation | Not given | 79.5 | | | | |
| Geraud ² | 247 | Riverside | 1 year | 53.3 | 26.7 | ... | 6.7 | 13.3 |
| Jones ⁶ | 30 | California physicians | 5 years | 8.0 | ... | ... | ... | 92.0 |
| Trussell ¹⁵ | 49 | Riverside | 2½-3 years | 90 Relapsed, had police trouble, or both. | | | | |
| | | | | 4 No relapse, no police trouble. | | | | |
| | | | | 6 Insufficient information to classify. | | | | |
| Diskind ^{1,15} | 344 | New York parolees | 2-36 months | 55.0 | ... | ... | ... | 45.0 |
| Hunt ¹ | 1,912 | Lexington | 1-4½ years | 90.1 | 3.3 | ... | ... | 6.6 |
| Lieberman ⁸ | 389 | Civil commitments to state hospitals | 1-3½ years | 18 Readmitted to a state hospital. | | | | |
| Duvall ¹ | 453 | Lexington | 5 years | 46.0 | ... | ... | | Vol., 25 Invol., 24 |

not, however, be assumed. It could be that each study is accurate and that the differences in their findings indicate that relapse rates vary widely, in association with factors to be inferred from characteristics of the samples studied.

One finding is well established by these studies; that is, when relapse occurs, it tends to occur quickly. Hunt reports that 90 per cent of the relapses which will occur take place within 6 months, and almost all of the rest within 2 years. The other studies which touch on this point confirm the finding, and none reports any evidence to the contrary.

The relapse rate of addicts, and the time within which relapse occurs, are of obvious importance in a practical sense, but the questions they answer are trivial, from the viewpoint of scientific theory. The questions which have theoretical importance—and equal practical importance—relate to the factors which distinguish the relapsed from the abstinent, and those who relapse quickly from those who abstain for fairly long periods. Such distinguishing factors, unfortunately, become difficult to identify when almost all of the subjects fall in one classification, and almost none in the other. Only a few of these studies even try to identify these distinguishing factors.

A few, however, are identified, and others can be inferred from the individual studies or from a comparison of the studies. The variables considered here are to be considered not as established predictors of relapse or abstinence, but as possible predictors, suggested by the 11 studies under consideration, which would be worth investigation in future studies.

1. **Sex.** Only half of the studies included women in the sample, and of these only Hunt and Duvall included enough for comparison to be meaningful. No difference in relapse rates by sex is reported by either, but certain differences by other variables held only for men.

2. **Age.** Hunt reports that, for men, those who were 30 years or older had lower read addiction rates than those under 30. This finding was again reported by Duvall. Diskind also reports tentative evidence that subjects over 25 abstained more frequently than the younger group. It may be noted that the two studies based

on Riverside patients, whose median age was about 18, report higher relapse rates than most of the other studies, which used older samples. Age may play some part, therefore, in explaining the differences in relapse rates.

3. Voluntary Status. Hunt found that, among patients 30 years or older, nonvoluntary patients had lower relapse rates than voluntary patients. Pescor's findings suggest that it may be post-hospital supervision, rather than nonvoluntary status, which affects relapse rates. He also reported lower relapse rates for parolees as compared with probationers. It will be noted, on the other hand, that Knight's subjects were voluntary, and Lieberman's and Jones's, while under some legal compulsion, were not convicted of crime, and all three studies report low relapse rates.

4. Race. Hunt reports that, among nonvoluntary patients under 30 years of age, whites had a lower relapse rate than Negroes. Gerard also found whites doing better than Negroes, with Puerto Ricans doing still better.

5. Length of Hospitalization. Hunt reports that, among voluntary patients under 30 years of age, those hospitalized 31 days or more had a lower relapse rate than patients hospitalized 30 days or less. Knight reports an average duration of hospitalization of 5.4 months for his abstinent patients, against 3.3 months for all patients in the sample. On the basis of his findings, Pescor recommended a comparatively short period of hospitalization, from 2 to 5 months.

6. Social Class. The lowest relapse rates are reported by Jones, for a group of physicians, and by Knight, for an upper-middle-class group which included many professionals.

Other variables of interest are noted in only one study each. Trussell reports that the number of posthospital arrests, and the number of new admissions to Riverside, varied:

1. Directly with recorded prehospitalization offenses
2. Directly with length of drug use prior to first Riverside admission
3. Inversely with the age at which marihuana or heroin was first used

Knight implies, but does not clearly document, a lower rate

of relapse for patients whose personality study revealed a predominance of psychoneurotic traits. Gerard reports a better post-hospital adjustment for patients who accepted a relationship with a therapist, and a worse one for those who, without accepting such a relationship, conformed to hospital regulations and expectations. Further, of those patients who predict, at the time of leaving the hospital, that they will remain drug-free, those whose reasons show ego development have a better posthospital adjustment than those whose reasons indicate repression, omnipotence, or denial.

One final hypothesis can be suggested on the basis of the 11 studies as a group. Six of them—Kuznesof, Hunt, Duvall, Trussell, Gerard, and Diskind—were done in New York City. These report six of the seven highest relapse rates or failure rates. This could mean that relapse is more probable in New York than elsewhere. In an unpublished pilot study done by this author a few years ago, it was found that the failure rate was higher, to a statistically significant degree, for parolees and probationer addicts in New York City than in the rest of the country. We know that relapse is triggered quite frequently by contacts with old addict friends, and we might speculate that such contacts are more probable, the larger the number of addicts in the community to which the subject returns.

DEFINITION OF RELAPSE AND ABSTINENCE

It is apparent from a close reading of these 11 reports that there is some variation in their definitions of relapse and abstinence, but few formal definitions are given. Hunt and Duvall use the classification of readdicted, irregular use, and abstinent, and define them as follows: readdicted means that the patient is using, or has used, narcotic drugs to the extent of at least one injection per day, for a period of 2 weeks; in irregular use, the patient used drugs to some lesser extent; abstinent means that he is not taking, and has not taken, any narcotics. Diskind clearly implies that a single use of drugs, preceded and followed by long periods of complete abstinence, was sufficient to classify a parolee as "reverted to the use of drugs." In those studies which relied, in part, on information obtained by questionnaire, it is obvious

that the effective definitions of the terms used must have been those of the respondents, allowing for the possibility that definitions and therefore classifications varied within several of the studies, as well as between them.

One point, however, is clear from those studies which define their terms. "Abstinence" refers to the entire period of time from the beginning to the end of the follow-up period, while "relapse" refers to a point in time, or a very brief span of time, within the total follow-up period. This is most clearly exemplified in the Hunt report. There, to be classified as abstinent, the subject had to remain drug-free for a period of 1 to 4½ years, depending on when he entered the follow-up. The readdicted group, however, can contain (1) some who used drugs for the entire period of time; (2) some who remained drug-free for over 4 years and then relapsed for 2 weeks just before the end of the follow-up; and (3) some who used drugs for 2 weeks soon after release from the hospital and then were abstinent for years through the remainder of the follow-up period. These three patterns are chosen to exemplify the extreme possibilities; the number of different patterns which could be identified is much larger.

It is fruitless to argue definitions. An investigator has the right to use any classification he considers useful, though he should, as does Hunt, make it clear to his audience what his definitions are. It is legitimate, however, to ask if the classification chosen is as useful as others would be. The classifications used in most of these studies are questionable in that they conceal as much or more information than they reveal; they group together, and thereby make seem identical, great differences in behavior.

The point can best be made by an improbable, but legitimate, speculation based on the Hunt report. Most people would feel that its finding of 90 per cent readdicted is discouraging. There were almost 2,000 subjects in the study, and the average length of time between their discharge from the hospital to the end of the follow-up period was certainly over 2 years. We have, then, roughly 4,000 man-years to be accounted for in periods of abstinence and drug use. The 90 per cent readdiction rate could mean, then, that about 3,600 man-years were spent using drugs,

an equally discouraging finding, if it were to be made. But taking the 2-week criterion for readdiction as about 2 per cent of the total follow-up period, the 90 per cent readdiction rate could also mean that as few as 72 man-years were spent using drugs, that over 3,900 of the 4,000 man-years were drug-free. Such a finding would be less discouraging.

The point, of course, is not to suggest that the true figure lies close to the latter extreme, nor that Hunt could have reported his findings in these terms. It is simply that this is a legitimate and useful question to ask of follow-up data, and future studies should make the attempt to answer it.

Addiction to opiates can be seen as a long and complex process, frequently involving alternating periods of use and of abstinence. The abstinence, in turn, can be broken into periods of enforced abstinence in institutions and periods of abstinence in the community, perhaps voluntary on the part of the subject and perhaps forced on him by circumstances. It is measures of these periods, in terms of drug-free months, or the ratio of this number to the months during which the subject could have been using drugs, which would be the meaningful variable in future follow-up studies.

This principle was first recognized by Gerard, who scaled drug use on a continuum so that, for example, use of marihuana alone or occasional use of heroin could be classed as improvement over previous addiction, and heavier use could be classed as deterioration. The later Trussell study is the first, however, to measure time on and off drugs, time on the street, and time in institutions. It then becomes evident that, even among those who relapsed, there are some who could be classed as much more successful than others.

Such differences tell us something at least as important as the fact that these subjects did relapse. In addition, they open up possibilities for research which are lost by the simple relapse-abstinence classification. Against them other differences, perhaps differences in treatment methods, can be measured, or predictions and theories can be tested.

Duvall carries the concept much further, by maintaining con-

tact with the subjects for a full 5-year period and reporting addiction status at three points in time, at 6 months, 2 years, and 5 years after discharge from the hospital. This approach succeeds immediately in establishing a fact which the other studies were unable, or failed, to look at. Duvall reports that only 12 of the 453 subjects were voluntarily abstinent for the full 5 years. The group which was voluntarily abstinent from narcotics at these three points in time increased from 9 per cent at 6 months to 17 per cent at 2 years, and 25 per cent at 5 years. At the end of the 5 years the percentage abstinent, either voluntarily or involuntarily, was actually slightly higher than the percentage readdicted.

One other point remains to be made on the classifications used in follow-up studies of addicts. It would be well to avoid complex classifications, in which relapse is grouped together with other criteria, like arrest or conviction, as "failure." It cannot be regarded as wrong to do this, and indeed studies like those of Kuznesof and Diskind are almost forced, to be comparable with other probation and parole statistics, to use such a complex classification. But it is possible, as Diskind does, to report the types of failure separately so that we can isolate relapse for study.

The Trussell report, on the other hand, devotes about 80 per cent of its discussion of findings to the 139 subjects who were located and found to have relapsed. Twenty per cent is devoted to the entire group of 247 subjects, for whom it is reported that 90 per cent had continued difficulties with narcotics or the police or both. The net effect of reading the report is, again, discouraging. Yet it remains true that as far as the report tells us, only 139 subjects are known to have relapsed, and the relapse rate for the entire group of 247 may be 56 per cent. And 56 per cent relapse rate would be regarded as encouraging by many.

This question of arrest as an indicator of failure has other aspects. There is the fact, for example, that in the disposal of the arrests reported by Trussell, 14 per cent were dismissed. Legally, this means that the persons arrested were innocent of the charge, or at least not proved guilty of it. We could be in the position, therefore, of classifying a subject as a failure simply because he had been arrested for something he did not do.

Another aspect is the purpose for which subjects are classified. Suppose, for example, the goal is to measure the effectiveness of treatment of addiction. Two patients are treated, both abstain from drug use, one returns to his previous occupation as a physician, and the second to his previous occupation as a shoplifter. The first is clearly a success; it is not equally clear that the second is a failure. But there is no need to tackle the philosophical questions involved. If the facts are reported, both the shoplifting and the abstinence, the investigator and the reader of his report can use the behaviors separately or together as criteria.

METHODS OF CLASSIFICATION

Abstinence and relapse are not simply facts which can be observed and recorded. They are conclusions, judgments, based on evidence. Further, the relapse-abstinence dichotomy or continuum—it can be handled either way—has an interesting quality. It is relatively easy to get firm evidence of relapse in a large proportion of cases. The patient returns to the hospital for further treatment or is observed to show withdrawal symptoms, or is arrested with narcotics in his possession and fresh needle marks on his arms, or admits to the investigator that he has been using narcotics regularly.²⁹

Equally firm and adequate evidence of abstinence is theoretically possible, if the subject is hospitalized under close observation in a drug-free environment and shows no withdrawal symptoms, or if he does not react positively to one of the antagonist drugs like Nalline, or if his urine tests negative for opiates. But none of the studies cited obtained such evidence of abstinence. A classification of abstinence in these studies, then, can mean only that the investigator has sought for evidence of relapse, is reasonably satisfied that he would have found this evidence if it existed, and has failed to find it. Proving abstinence becomes proving a negative, and this is notoriously difficult.

Further, most of these types of strong evidence of drug use or abstinence establish the classification for only a brief period of time. If the urine test is negative, it remains possible that the subject was using drugs up to a few weeks, or even a few days,

before the date of the test. If withdrawal symptoms are observed, it may be that the drug use began only a few months, or less, before the date of the observation. It is much more difficult, therefore, to establish the pattern of use and abstinence over a long period of time. This may account, in large part, for the tendency in these studies to accept relapse at one point in time as the final basis for classification. If the subject has been out of the hospital for 5 years, and claims abstinence for the past 4 years, but admits use for the first year after release, the investigator can comfortably count him as relapsed and have no worries about what kinds of confirmation he would need to accept the claim of abstinence for 4 years.

Although firm evidence of relapse may be available for a fairly large proportion of the sample, there will also be many cases in which it is not found, and the investigator will have to base his classification on more tenuous evidence. Such evidence is exemplified in the signs of relapse noted by Diskind: "... the partaking of sweets and soda, blood stains on shirt sleeves and in the bathroom, drinking ice water to excess, powder or salve on arms, irregular employment pattern, and difficulty in waking the parolee in the morning. . . ." ¹

Those who have worked with addicts will agree that such signs do, or can, indicate relapse. They will also agree that the signs vary in value, with bloodstains on sleeves, for example, indicating somewhat more than drinking ice water or sleeping late in the morning.

There is an obvious possibility of error in classifying a subject as relapsed on the basis of such evidence. The classification of abstinence or relapse, then, is not analogous to a reading from a calibrated scale by a white-coated scientist in a laboratory. It much more closely resembles the process by which a physician makes a diagnosis, taking into account a wide variety of symptoms and signs, and giving to each the weight which his experience suggests. It even more closely resembles the process by which a juryman votes "guilty" or "not guilty" on the basis of sometimes conflicting testimony, some of which he chooses to believe, and some to discount.

In short, the classification of subjects as relapsed or abstinent is subject to question. It is a problem of measurement, and the usual questions of the reliability and validity of the measurement may be raised. In terms of follow-up studies of addicts, the questions we may ask include at least these:

1. What kinds of evidence of use or abstinence did the investigator obtain?

2. When different degrees of evidence were available, in what proportion of cases were strong and weak evidence used for the classification?

3. What procedural steps were taken to move from the evidence available to the classification as relapsed or abstinent?

4. What steps were taken to measure the degree of reliability of the classification?

5. How valid, how correct, is the classification?

A major flaw in almost all of these studies is that they give the reader little or no information to answer these questions. This lack of information is disturbing because it may imply that the investigators were not aware of the legitimacy and importance of the questions and, therefore, did not take the necessary precautions to insure against errors and bias.

On the point of the data available to the investigators, there is a very wide range. At the one extreme, Diskind's parole officers saw their subjects weekly for 9 months, and twice a month after that, plus interviewing family members regularly, and probably employers and other informants. In addition, they received notice automatically if the parolee was arrested. Kuznesof's contacts were of the same type, though less intensive and frequent. At the other extreme, Jones gives no information on what data he had, and Lieberman got only reports of arrests and rehospitalizations, and those only from one state, with no firm knowledge that his subjects had remained in that state.

The remaining studies all obtained information from sources of varying value, but do not tell us in how many cases they had strong evidence and in how many it was more dubious. In Pescor's study, for example, it would be helpful to know in how many cases his classification is based on readmission to the hospital, in how many on detailed reports from probation officers, and in how

many on questionnaires alone. In the study reported by Hunt, many of the subjects were interviewed and some of them regularly over a period of years. In other cases, however, there may have been no more than one or two telephone contacts with a relative or other source of information. The findings of the study will obviously carry more weight if most cases fall in the first, rather than the second, category.

Hunt was aware of the importance of this point. The first of the three major questions he lists as the purposes of the study is "Can contact be achieved?" with the study subjects. But the only answer he gives is that "some degree of contact was achieved with 1,881, or 98.4 per cent."⁴ This is not a satisfactory answer. What we should be told is how much contact was achieved with how many patients.

After the data are gathered, the possibility of error exists in the process of using them to classify the subjects. In this step, Lieberman had no problem. His data consisted in reports of arrest or hospital admission, and no judgment was involved in classification. Trussell gives no specific information on the classification procedure, but seems to have found both rehospitalization and admission of drug use for almost all subjects he classified as continuing to use drugs, and on the others he used the fact of arrest as the basis for classification. Gerard alone describes how the judgment was reached, including the use of formally defined scales, with examples of where subjects should be placed, and training of raters. Diskind does not specifically discuss the problem involved in reaching a judgment, but does mention a program in which project personnel were trained to deal with the problem.

These four studies, then, either had no classification problem or took steps to handle it, and the reader can probably assume that the reliability of their classifications is high, even though not formally measured. The remaining studies are more difficult to evaluate in this respect, because they give no information on the point. To the extent that they based classification on questionnaire responses, the reliability of the classification is probably high, but its validity more dubious. To the extent that other data were used, the validity may increase, but the reliability suffer.

Among the seven other studies, only Hunt touches on the ques-

tion of classification of data. Efforts were made to weight the various kinds of information obtained, to array them

. . . in a series with consistently increasing validity. All of these attempts proved fruitless and were abandoned. . . . During the final years of the study, the chief of the follow-up team reviewed the records of all patients and was responsible for determining the final classification of each patient in the study. If there were any doubts about the diagnosis of readdiction, the patient was classified as an irregular user or as abstinent.⁴

Here, then, the problem was recognized, and it would have been prudent to have the classification made independently by one or more additional judges. Then their agreement could have been measured. Instead, the reader is asked to accept on faith what could have been based on measurement. Since the Duvall study made use of the procedures described by Hunt, the same criticism applies to it.

A measure of validity implies a comparison with an independent criterion. In these studies, no such criterion was available. A practical procedure, however, and one which might be followed in future studies, would be to break down the classifications by the weight of evidence on which they are based. The report could show, for example, how many classifications were based on strong, moderate, and weak evidence, and give examples for each group. The reader would then be in a position to evaluate the findings in terms of his agreement or disagreement with the investigator's judgment.

SUGGESTIONS FOR FUTURE FOLLOW-UPS

The major points made above may be summarized, and a few minor points added, in terms of suggestions for future follow-up studies.

More attention should be paid to the problem of what evidence is sufficient to classify the subjects. A useful tool would be an objective index of use at the time contact is made with the subject. The use of Nalline testing, in California and Illinois, and of urine testing for parolees, in Colorado and Wisconsin, will be extremely valuable. In these programs, the subjects can be required to submit to the testing, and we may not be able to generalize from

these studies to voluntary patients. In a study we are conducting in Kentucky, however, only 1 subject of the first 81 located refused to give a urine specimen, and the early experience with a similar study in Puerto Rico suggests that the refusal rate there will also be low.

The validity of findings will also be greatly increased by repeated contacts with subjects, as in the parole programs mentioned above. This would pose practical problems, however, for studies which cover a longer follow-up period. The design of such studies is almost automatically limited to a one-contact approach. The problem of what evidence to accept for classifying patients as abstinent over past periods of time is a serious one, and one which we are not sure we can solve in our Kentucky study, which covers a 25-year period for some subjects. The way these problems are handled, however, can be described in the report, so that readers can credit or discount the findings to the extent they consider justified.

The relapse-abstinence dichotomy should be replaced by an emphasis on identifying periods of use and abstinence and the circumstances associated with these. It will be useful to identify those addicts who gave up drug use for long periods of time, even though they may later have relapsed, and see if the factors which account for these periods can be isolated. Their adjustment in other areas while abstinent should also be studied. If an addict gives up narcotic use, does he automatically become a good husband, father, and citizen, or does he perhaps shift to alcohol or barbiturates, or become a more efficient criminal when the pressures and dangers of drug use are avoided?

A new point to be made is the importance of reducing the number in the "unknown" category to the absolute minimum. In Pescor's study, for example, 40 per cent are classed as "addiction status unknown." But it was known that none of these had returned to the hospital, and it was known for those subjects who had been prisoners or probationers—73 per cent of his sample—that no notice of subsequent arrest was received from the FBI. The number of later arrests for this group, therefore, must have been low.

So, for most of the unknowns in the study, for about fifteen hundred individuals, we actually know two facts which do not establish abstinence, but are consistent with it. If a patient left the hospital determined to give up drug use, and broke away from his old ties to help him do this, where would he be classified? He would not return to the hospital, he would not be arrested, and a questionnaire sent to his old address might well not reach him. He could fall only in the unknown category. It may, therefore, be that the very subjects who are hardest to locate and classify are those most likely to be abstinent.

That this is more than a possibility can even be demonstrated from the Trussell report. Slightly under half of the subjects were easily located, because they were institutionalized again, and almost all of these had relapsed. The few exceptions had probably not been addicted on their first hospitalization. About 90 per cent of the subjects were identified in an intensive search of records, and of these, almost all could be classed as having continued difficulty with narcotics or the police or both. Twenty-nine subjects, however, were not located by these means. They could have been written off as "unknown." But by virtue of an extra effort, sufficient data for classification were obtained on 19 of these, and 11 of the 19 were found not to have relapsed, though two of them had police difficulty without drug use. In short, the relapse rate in this hard-to-locate group was much lower than in the easily located subjects, and indeed almost all of the known abstinent cases in the study are accounted for by this group.

CONCLUSIONS

What, then, can we say we have learned from these follow-up studies? Most of them do seem to establish that high percentages of their samples relapsed to drug use, but this is true only for a highly restricted definition of relapse. If one believes that most addicts, after a period of treatment or enforced abstinence, relapse to drugs and continue to use drugs, or that addicts spend most of their time outside of institutions using drugs, this may be true, but not one of these studies establishes it as true, or even indicates that it is probable. They offer us no evidence on this,

except for the Duvall study, which points in the opposite direction, to less use of narcotics as more time elapses.

On the face of it, these studies indicate a wide range in relapse rates. It would be possible to take those at one extreme, for example, those that report low relapse rates, and by legitimate criticism show that so many sources of error are possible that the low relapse rates need not be accepted. But the same criteria would bring the high relapse rates reported in other studies equally into question. It would seem safest to accept all of the studies at face value, as indicating variations which are not yet explained. Whatever the definition of relapse one prefers, and whatever the rates of relapse may be, these studies strongly indicate that there are differences in relapse for different subgroups of addicts.

Many readers of these reports have uncritically accepted them as painting a discouraging picture, as indicating that past and present programs for addicts have been ineffective. This, it would seem, is a dangerous conclusion, not so much because it might discourage those who are now working with addicts, but because it might make experimental programs, using slightly different classifications as criteria, seem more effective than they actually are. The safest general conclusion is that far more knowledge of relapse, and the factors associated with it, is needed than we have obtained from the studies done to date.

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- are computed from table 11, p. 26, and the coding instructions on p. 59. These show that 16 boys returned to daily use of opiates, 8 used alcohol to excess and/or used marihuana, and perhaps used narcotics irregularly, 4 used no opiates, and 2 were unknown.
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jects as relapsed, but it is probable that most of them classed at least some subjects as relapsed solely on the basis of the subject's admission of drug use.

At least two subjects in the Hunt study, however, on later admissions to Lexington, told staff members they had reported relapse to research staff earlier than it actually occurred, as a quick and easy way to get rid of the researcher. Whether this is true is unknown, but it clearly represents a possibility. Even when the investigator has the admission of drug use, therefore, it would be desirable to have some confirming evidence before classifying the subject as relapsed.

